XVIII. *Concerning the Latitude and Longitude of the Royal Obſervatory at* Greenwich; *with Remarks on a Memorial of the late M.* Caſſini de Thury. *By the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read Feb. 22, 1787.

MEMOIRE

Sur la jonction de Douvres à Londres.

Par M. CASSINI DE THURY, Directeur de l'Obſervatoire Royal; de la Société Royale de Londres, &c.

IL eſt intéreſſant pour le progrès de l'aſtronomie que l'on connaiſſe exactement la différence de longitude et de latitude entre les deux plus fameux Obſervatoires de l'Europe; et quoique les obſervations aſtronomiques faites depuis un ſiècle offrent un moyen aſſez exact pour parvenir à cette recherche, il parait cependant que l'on n'eſt point d'accord ſur la longitude de Greenwich à onze ſeconds près, et ſur ſa latitude à quinze ſecondes.

L'on a reconnu par les opérations trigonométriques exécutées en France, au Nord, et au Perou, que ſur l'étendue d'un degré du méridien ou de 57 mille toiſes, l'on ſe trompait à peine de dix toiſes, ce qui a été prouvé par des baſes meſurées à l'extrémité des ſuites de triangles; ainſi ſur la diſtance de Douvres à Londres, qui eſt de 49800 toiſes ou environ, on ne pourrait ſe tromper de 120 toiſes, qui repondent à onze ſecondes en longitude.

M. CASSINI a déjà publié, dans le livre de la Méridienne Vérifiée, ses opérations par lesquelles l'on a déterminé la distance de Calais à la grosse tour de Douvres de 18241 toises par un premier triangle, et de 18243 toises par un second triangle; on aurait cette distance avec une plus grande exactitude en observant les angles conclus à *Douvres, qui sont fort aigus.* M. CASSINI a découvert des côtes de France plusieurs objets sur les côtes d'Angleterre, qui seront visibles de la tour de Douvres; et sur cette première base on établirait une suite de quelques triangles jusqu'à Londres, dont le nombre et la grandeur dépendent de l'exposition des objets compris dans la direction de Douvres à Londres.

M. CASSINI ne doute point que ce projet ne soit agréé d'un Souverain qui aime les sciences, qui non content des découvertes du celèbre COOK vient d'ordonner un second voyage autour du monde, et que la Société Royale ne charge un de ses Membres de l'exécution; et dans le cas où ses occupations l'empêcheraient de s'y livrer, qu'elle ne permît à M. CASSINI de s'en charger. L'honneur qu'elle lui a fait de l'associer à un corps aussi respectable serait un titre pour lui accorder sa confiance. M. CASSINI a profité du voyage du Roi en Flanders en 1748 pour joindre les triangles de la méridienne à ceux de SNELLIUS en Hollande; en 1762 il a prolongé la perpendiculaire de Paris jusqu'à Vienne en Autriche. La branche qui s'étendra jusqu'à Londres sera la troisième, et formera la jonction des deux plus belles villes de l'Europe.

THE

THE preceding Memorial of the late M. CASSINI DE THURY was put into my hands by Sir JOSEPH BANKS, our Preſident, on the 28th of April, 1785, deſiring me at the ſame time to give an anſwer to it. Happy if I can ſolve the doubts entertained by the late Royal Aſtronomer of France concerning the latitude and longitude of this Royal Obſervatory, and at the ſame time do juſtice to the memories of my learned predeceſſors, and to myſelf, I ſhall give an account of the principal operations that have been performed here for aſcertaining thoſe points, and then add my own remarks to elucidate the ſubject and reconcile the difficulties in queſtion.

Had Dr. BRADLEY lived longer, for the benefit of aſtronomy, to publiſh his valuable obſervations, or had they been ſince publiſhed by another hand, which unfortunately they hitherto have not, theſe remarks might have been unneceſſary, and perhaps even the occaſion for them might never have occurred; as it would have then appeared upon what foundation the latitude of this Obſervatory had been eſtabliſhed, and what differences of meridians between Greenwich and the other principal Obſervatories of Europe reſulted from the obſerved eclipſes of Jupiter's ſatellites and other celeſtial phænomena.

However, having formerly been appriſed by Dr. BRADLEY himſelf of ſeveral particulars of moment relative to his obſervations, and particularly of the method which he uſed for ſettling his latitude and refractions, after he became poſſeſſed of the new inſtruments in 1750, and being aſſiſted with ſome of his manuſcript calculations, with the addition of my own obſervations, I flatter myſelf I can throw the light wanted on the

 queſtion,

question, and obviate the principal difficulty, that relative to the difference of latitude of Greenwich and Paris, and reduce the difference of meridians within smaller limits, notwithstanding Dr. Bradley's original observations had been removed from this Observatory, in which they were made, before I came here, and have not yet been restored to it.

Dr. Bradley having been furnished by Government in the year 1750 with a brass mural quadrant of eight feet radius, constructed by that excellent artist Mr. John Bird, an instrument far superior to any before used in the practice of astronomy, assiduously observed the pole star and other stars lying to the north of the zenith with it for upwards of three years, and then removed it to the opposite side of the wall, making it change place with the iron quadrant of the same radius constructed by Mr. Graham, likewise an excellent instrument, though inferior to this, and commenced a regular series of observations of the sun, planets, and fixed stars, which have been ever since continued in the same manner. Moreover, the temperature of the air, shewn by the barometer and thermometer, is affixed to each observation; and the zenith point of the quadrant settled from time to time by the help of a zenith sector of 12½ feet radius, turned alternately contrary ways, the same with which Dr. Bradley had before made his two useful and admirable discoveries of the aberration of light and the nutation of the earth's axis.

By the observations of the pole star and other circumpolar stars, above and below the pole, Dr. Bradley got the apparent zenith distance of the pole; by the apparent and equal zenith distances of the sun at the two equinoxes, having at the same time opposite right ascensions, as found from comparing his observed transits over the meridian with those of

 fixed

fixed ſtars, after the manner uſed by Mr. FLAMSTEED for deducing the right aſcenſions of the fixed ſtars, he found the apparent zenith diſtance of the equator, which leſſened by parallax and added to the apparent zenith diſtance of the pole gave a ſum leſs than 90° by the ſum of the two refractions belonging to the pole and meridian zenith diſtance of the equator. But he remarked, that the difference of refractions, belonging to theſe zenith diſtances, would come out the ſame within 2 or 3″ by any of the beſt tables then extant, whether deduced ſolely from obſervations, or partly from obſervations and partly from theory. The ſum and difference of refractions anſwering to the pole and equator being thus given, the refractions themſelves are given, the greater of which added to the apparent zenith diſtance of the equator gives the latitude of the place, and the leſs refraction added to the apparent zenith diſtance of the pole gives the co-latitude.

He afterwards, from the conſideration that the refractions at the pole and equator may be taken without ſenſible error as the tangents of the zenith diſtances, according to Mr. THOMAS SIMPSON's theory of refractions in his Mathematical Diſſertations, divided more accurately the ſum of the refractions at the pole and the equator into the juſt parts anſwering to each zenith diſtance, and thereby found the latitude with more exactneſs. In this manner he found the latitude of the Royal Obſervatory to be 51° 28′ 39″½, and the mean refraction at 45° 3′ to be 57″, the barometer ſtanding at 29,6 inches, and the thermometer of FAHRENHEIT's ſcale at 50°.

But, not to let a matter of ſo much conſequence reſt on my aſſertion or memory, when further proof can be given of it, I have by me, in the hand-writing of Dr. BRADLEY, among other particulars, his calculations of the latitude of the Obſer-

vatory from his observations, according to the manner above explained; in which he first states it at 51° 28′ 38″, and finally more correctly in these words. "The apparent zenith "distance of the equator, by the mean of 20 observations in "1746-47, 51° 27′ 28″. The mean apparent distance of the "pole, by the observations made between 1750-52, 38° 30′ 35″. "Sum 89° 58′ 3″. Sum of refractions 1′ 57″. Polar refrac-"tion 0′ 45″½. Equatorial refraction 1′ 11″½. Latitude "51° 28′ 39″½. Co-latitude 38° 31′ 20″½."

The latitude of the Observatory being thus settled, as well as the quantity of refractions for all stars passing the meridian between the pole and the equator, Dr. BRADLEY readily inferred from his observations the true distance of all such stars from the north pole, which, compared with their zenith distances observed below the pole, gave the refractions at those lower altitudes. Finally, by comparing the refractions together observed in extreme degrees of heat and cold, he deduced the law of their variation as affected by heat and cold; and thus at length he inferred his elegant rule for determining the refraction in all circumstances, that it is to 57″, in the direct compound ratio of the tangent of the apparent zenith distance lessened by 3 times the refraction to the radius, and of the height of the barometer in inches to 29,6 inches, and in the inverse ratio of the degree of height of FAHRENHEIT's thermometer increased by 350 to 400.

But it may be proper to confirm this rule for refractions also from the same manuscript of Dr. BRADLEY, which I before cited for confirming the latitude, by the following passage, which immediately follows the other. "Suppose the "mean refraction at 45° 3′ = 57″, and $y = 350$; then "$y + t$: bar. :: 77″ : refr. at 45° 3′.

"Rad.

"Rad. : tan. ZD :: 3′ : *m*..... Rad. : $\overline{\text{tan.ZD} - m}$ :: refr. at 45°3′ : *r*
" [the refraction required]." It is eaſy to ſee that this rule agrees with the other: for putting $t = 50$, and barometer $= 29{,}6$, the firſt analogy, putting the barometer down in tenths of an inch, is $350 + 50 = 400 : 296 :: 77'' : 56'',98$ for the refraction at 45° 3′, or 57″ within $\frac{1}{500}$th of a ſecond. The ſecond analogy ſerves to give the treble refraction nearly, called *m*. Whence it is evident, the laſt analogy coincides with the rule above given.

This valuable rule was firſt communicated by myſelf to the public in vol. LIV. of the Philoſophical Tranſactions, p. 265. and in p. 49. and 129. of the firſt edition of Tables requiſite to be uſed with the Nautical Almanac, together with a Table of the mean Refractions deduced from it, with the firſt Nautical Almanac, that of 1767, publiſhed by order of the Commiſſioners of Longitude in 1766; and again, at page the 5th of the Explanation and Uſe of the Aſtronomical Tables, annexed to the firſt volume of my Obſervations made at the Royal Obſervatory from 1765 to 1774, publiſhed by order of the Preſident and Council of the Royal Society, with two tables in that work, containing the mean refractions and decimal multipliers for reducing them to any given temperature of the air indicated by the barometer and thermometer. The words in page the 5th of the ſaid preface are as follows. " The
" aſtronomical refractions and latitude of the Obſervatory were
" ſettled with the greateſt accuracy by Dr. BRADLEY, from his
" obſervations of the circumpolar ſtars, with the braſs mural
" quadrant, during the three years that it was turned to the north,
" and of the ſun and ſtars in the ſubſequent years after it was
" removed to point to the ſouth. The following elegant rule
" was

"was the result of his observations, that the refraction at any "altitude is to 57 seconds, in the direct compound ratio of the "tangent of the apparent zenith distance lessened by 3 times "the refraction to the radius, and of the altitude of the baro- "meter in inches to 29,6 inches, and in the reciprocal ratio "of the height of FAHRENHEIT's thermometer increased by "the number 350, to the number 400. Tables XXII. and "XXIII. were adapted to this rule; the first containing the "mean refractions answering to 29,6 inches height of the "barometer and 50 degrees height of the thermometer; and "the second table containing decimals for multiplying the "mean refraction in order to find the correction, which applied "to it will give the actual refraction, the same as would have "been produced by the rule with somewhat more trouble. "Dr. BRADLEY supposed the horizontal parallax of the sun "$10\frac{1}{3}$ seconds, in the calculations from which he inferred the "refractions; and I have been informed, that he determined "the latitude of the Observatory $51^\circ\ 28'\ 39''\frac{1}{2}$. But, had he "made use of the true parallax $8''8$ or $8''\frac{3}{4}$, as found by the "two late transits of Venus over the sun, he would have made "the refraction at the altitude of 45° to be $56''\frac{1}{2}$ instead of "$57''$, and the latitude of the Observatory exactly $51^\circ\ 28'\ 40''$ "instead of $51^\circ\ 28'\ 39''\frac{1}{2}$. But his rule for refractions can- "not be corrected for all altitudes, without examining his ob- "servations of refractions made at various times."

On comparing this extract with M. CASSINI's Memoir, I cannot but express my surprise, that he should not have adverted to a passage containing so direct an application to the grounds of his Memorial, in a publication of such notoriety, and of so old a date as 1776; had he done so, I cannot but think, he would never have hazarded such an opinion as that advanced by him in

in his Memoir of an uncertainty of 15″ in the latitude of Greenwich; but he might have been induced to believe, that the latitude of this place had been well determined.

For further confirmation of the certainty of the aſtronomical refractions, and latitude of the Obſervatory, as ſettled by Dr. BRADLEY, it may be proper to add, that the Greenwich braſs mural quadrant underwent a trial, which all aſtronomical inſtruments ought to be ſubmitted to, but which very few ever have been, on account of the difficulty and nicety of the operation, namely, an examination of the total arc; when it was found by Dr. BRADLEY to be an accurate quadrant, the arc appearing at one trial to differ only a fraction of a ſecond from 90°, and another time, after an interval of above ſix years, to be a perfect quadrant. See p. 24. of BIRD's Method of conſtructing Mural Quadrants, publiſhed by the Board of Longitude in 1768. In like manner he had before examined the total arc of the iron quadrant, firſt put up by Mr. GRAHAM, for the uſe of Dr. HALLEY, in the year 1725, by means of a level, and found it to be 16″ leſs than a quadrant. See BIRD's Method of conſtructing Mural Quadrants, p. 7. and Memoires of the Royal Academy of Sciences at Paris, for 1752, p. 424. But this quadrant was, in the year 1753, re-divided by Mr. BIRD, and, in this reſpect, probably rendered as accurate as the other. See BIRD's Method of conſtructing Mural Quadrants, p. 24.

Dr. BRADLEY made a curious uſe of the new ſet of diviſions, ſoon after they were laid upon the quadrant, to re-examine the error of the total arc laid down originally by Mr. GRAHAM (which by the plumb-line and level he had found to be 16″ leſs than a quadrant in 1745) according to the following paſſage contained in the manuſcript before cited.

"August

"August 12, 1753, I measured with the screw of my mi-
"crometer the difference of the arcs (of $\frac{64}{96}$) as set off by
"Mr. GRAHAM originally, and by Mr. BIRD when he put
"on a new set of divisions upon the old quadrant, and I
"found that Mr. GRAHAM's arc was less than Mr. BIRD's by
"$\frac{8}{40}$ divisions of my micrometer, which to a radius of 96 inches
"answers to 10″,6; so that the whole arc of 96 differs from
"a true quadrant 15″,9, which is the same difference that I
"formerly found by means of the level, &c."

Let me further add, that Dr. BRADLEY had informed me, that he had found the same refractions, latitude of the Observatory, and obliquity of the ecliptic, by both quadrants, making a proportionable allowance, in the use of the iron quadrant, for the error of 16″ in the total arc in proportion to the zenith distance of the object before it was new divided.

The Rev. Dr. HORNSBY, F. R. S. Savilian Professor of Astronomy at Oxford, to whose care Dr. BRADLEY's original observations have been committed, in order to their being printed and published, having favoured me with calculations of the latitude of the Royal Observatory from observations of the pole star made with both quadrants, from a manuscript of Dr. BRADLEY, I think it proper to give it a place here, not only as a very curious paper, but also as strongly confirming the latitude of this place before stated.

Transcribed from a loose Paper of Dr. BRADLEY.

"The mean zenith distances of the pole star above and below
"the pole, corrected by refraction, aberration, &c. and re-
"duced to January 1751, O. St. as collected from the obser-
"vations made after the new quadrant was balanced, Nov.
"24, 1750.

Number

Number of obſer-vations.	Above the pole.	Number of obſer-vations.	Below the pole.	to
	° ′ ″		° ′ ″	
23	36 29 46,66	8	40 33 2,95	Jan. 15, 1751
22	45,42	26	3,37	Aug. 27, 1751
23	45,13	27	2,14	May 4, 1752
19	44,63	28	1,67	Nov. 10, 1752
28	45,00	23	1,74	May 31, 1753
9	44,59	8	1,54	July 26, 1753
124	36 29 45,24	120	40 33 2,24 36 29 45,24	

	77 2 47,48
	38 31 23,74
Error of collimation	1,74
Co-latitude	38 31 22, 0

° ′ ″
77 1 15,8

	38 30 37,9	Refraction.	° ′ ″	° ′ ″
Collimation	1,4	42″ +	36 29 2½	4 3 10,8
Refraction	+ 45,5	49 −	40 32 13½	+ 6,6
	38 31 22,0	6½	4 3 11	4 3 17,4
By new quad.	51 28 38,0	Refraction	+ 6½	
			4 3 17½	
			2 1 39 – the mean diſtance from mean pole Jan. 1751.	

The apparent zenith diſtance of the pole, by the mean of 310 obſervations, is

° ′ ″
38 30 36 allowing − 2″ for the error of the line of collimation.
Refraction + 45½

38 31 21½ Co-lat. 38° 31′ 21½″ by the new quadrant.
Latitude 51 28 38½ 38 31 18½ by the old quad. new diviſions.

Apparent zenith diſtances of the pole obſerved with the iron quadrant.

1753	Apparent zenith diſtance of the pole.	Barom.	Thermom. in.	Thermom. out.
	° ′ ″	Inches.	°	°
Sept. 13	38 30 40,1	30,10	64	65
23	41,5	29,88	61	60
Oct. 2	42,5	29,67	56	53
5	41,0	30,02	57	57
11	41,1	29,48	61	57
19	40,2	29,82	50	43
31	40,8	29,65	42	34
Nov. 5	42,1	29,69	45	39
16	40,5	29,39	42	35
19	42,2	29,59	40	35
20	40,9	29,84	40	33
24	40,0	30,00	43	35
29	40,0	29,80	38	30
Dec. 3	39,3	30,16	39	33
8	37,4	30,06	35	27
17	41,5	29,50	50	49
30	38,0	30,11	33	22
Mean	38 30 40,5	29,81	47	$41\frac{1}{2}$
Refraction	+ 46,4			
Col.	− 8,4			
Pol. corr.	38 31 18,5			

So far the manuſcript.

Thus the latitude by the braſs quadrant being $51^\circ\ 28'\ 38''\frac{1}{2}$, and by the iron quadrant with new diviſions $51^\circ\ 28'\ 41''\frac{1}{2}$, the mean by both quadrants is $51^\circ\ 28'\ 40''$, or only half a ſecond greater than ſettled in another manner, according to the manuſcript of Dr. BRADLEY in my poſſeſſion. Alſo the apparent zenith diſtance of the pole with the mean refraction $45'',4$ being $38^\circ\ 30'\ 36'',1$ by the braſs quadrant, and $38^\circ\ 30'\ 33'',1$, by the iron quadrant, the mean by both is

38°

38° 30′ 34″,6, or only $\frac{4}{10}$ths of a ſecond leſs than by Dr. BRADLEY's manuſcript cited before.

On my promotion to the Royal Obſervatory in 1765, finding its latitude to have been ſo accurately ſettled by Dr. BRADLEY before me, I might have thought myſelf diſpenſed from making any particular or very laborious obſervations for that purpoſe; however, I confirmed it by my own obſervations to great nearneſs, *viz.* within 1 or 2″, at the ſame time that I was eſtabliſhing a new catalogue of the principal fixed ſtars, continually obſerved here for ſettling the right aſcenſions of all other celeſtial objects with the tranſit inſtrument. The reſult in brief is as follows.

I firſt ſettled the relative right aſcenſions of about 30 of the brighteſt fixed ſtars, and lying neareſt the equator, by a great number of obſervations with the tranſit inſtrument, referring them to α Aquilæ as the fundamental ſtar, whoſe right aſcenſion I aſſumed from Dr. BRADLEY's determination. Hence, by obſerved tranſits of the ſun and the ſame ſtars, in the ſpring and autumn, when his daily motion in declination was at leaſt 16′ or two-thirds of the greateſt, I inferred the ſun's right aſcenſions relative to the right aſcenſions of thoſe ſtars ſettled in the manner juſt mentioned. Alſo from the ſun's obſerved zenith diſtances taken with the braſs mural quadrant on the ſame days, and corrected by refraction, parallax, and error of line of collimation, with Dr. BRADLEY's obliquity of the ecliptic, and latitude of the Obſervatory, I computed the ſun's declinations, and thence the right aſcenſions correſponding to them.

Now, if the aſſumed right aſcenſion of α Aquilæ, and thence thoſe of the other ſtars were affected with ſome ſmall error, as might be ſuppoſed, the ſun's right aſcenſions deduced from the obſerved tranſits would differ the ſame way from the

truth

truth at both ſeaſons of the year, *viz.* by the unknown error of the aſſumed right aſcenſion of α Aquilæ; but his right aſcenſions inferred from his obſerved zenith diſtances would be affected contrary ways at the two oppoſite ſeaſons of the year, by the unknown errors in the refractions, parallaxes, latitude of the place, and obliquity of the ecliptic. Hence, the mean of the two corrections of the ſun's right aſcenſion, found from the obſerved declinations about the vernal and autumnal equinox, would be the true correction of the aſſumed right aſcenſion of α Aquilæ; and the difference of the ſame corrections would, by an eaſy calculation, ſhew how much the computed declinations were too great or too little for the truth, and conſequently what the true declinations were, and what the true zenith diſtance of the ſun was, when in the equator, or the latitude of the place, on ſuppoſition that Dr. BRADLEY's refractions were truly ſtated; for any ſmall uncertainty in the obliquity of the ecliptic, as ſtated by him, could not affect this reſult, which was deduced equally from obſervations of the ſun in north and ſouth declination, when the ſame error of the obliquity would affect the ſun's right aſcenſions deduced from the obſerved declinations contrary ways. I took the ſun's parallax from the 24th of my tables annexed to my obſervations, conſtructed upon a horizontal parallax 8″,84, which I had deduced from the obſervations of the firſt tranſit of Venus, that in 1761, and differing inſenſibly from 8″¾, which I deduced from the obſervations of the total durations of the tranſit between the internal contacts obſerved at Wardhus and Otaheite in 1769, conſequently more correct than the horizontal parallax of 10″⅓ uſed by Dr. BRADLEY. It is alſo evident, that the true zenith diſtance of the equator thus found, diminiſhed by Dr. BRADLEY's mean refraction, will be the apparent

zenith diſtance of the equator, affected only by the mean refraction.

I ſhall now give the apparent zenith diſtance of the equator, and the true latitude of the Obſervatory, reſulting in this manner from my obſervations of ſix years, from the autumnal equinox of 1765 to that of 1771, in which I allowed 0″,9 for the correction of the error of the line of collimation, additive to the obſerved zenith diſtances, as I found from a reviſion of my calculations of the zenith diſtances of ſtars taken with the mural quadrant, compared with the like taken with the zenith ſector in 1768.

Years of the obſervations.	Number of days of obſervations.	Apparent zenith diſtance of equator, taking the ſun's horizontal parallax 8″,8.	True latitude of the Obſervatory, according to Dr. BRADLEY's refractions and the ſun's horizontal parallax 8″,8.
Autumnal equinox of 1765 and vernal of 1766	52	51° 27′ 28″,6	51° 28′ 40″,1
Autumnal equinox of 1766 and vernal of 1767	38	30,4	41,9
Both equinoxes of 1768 — —	46	32,2	43,7
Both equinoxes of 1769 — —	48	28,7	40,3
Both equinoxes of 1770 — —	20	29,3	40,8
Both equinoxes of 1771 — —	42	29,7	41,3
Mean from ſix years obſervations — —		51 27 29,8	51 28 41,3
Mean found by Dr. BRADLEY, with ☉'s horizontal parallax 10″⅓ — —		51 27 28	51 28 39,5
But if 1″,2 be added to reduce them to the ☉'s horizontal parallax 8″,8, Dr. BRADLEY's reſult will be changed to —		51 27 29,2	51 28 40,7
Differing from my determination above only		0,6	0,6

In further confirmation of the latitude of the Obſervatory, I ſhall now adduce eight years obſerved zenith diſtances of the ſun in the ſolſtices, being deduced from a number of obſervations taken at and near the ſolſtices, and corrected for line of collimation, refraction, parallax, and nutation.

Years

Years of observations.	Summer solstitial zenith distance reduced.	N° of days of observations.	Winter solstitial zenith distance.	N° of days of observations.	Half sum or latitude of the place.	Half difference or obliquity of the ecliptic.
	° ′ ″		° ′ ″		° ′ ″	° ′ ″
1765	28 0 30,2	6	74 56 46,1	5	51 28 38,2	23 28 8,0
1766	33,6	7	48,8	2	41,2	7,6
1767	32,0	6	uncertain from an accident.	—	— —	— —
1768	29,7	6	44,9	7	37,3	7,6
1769	31,8	8	46,2	5	39,0	7,2
1770	30,7	7	44,1	6	37,4	6,7
1771	32,0	8	42,2	9	37,1	5,1
1772	31,8	12	Line of collimation altered by applying an achromatic object-glass to the telescope.			
Mean	28 0 31,5		74 56 45,4		51 28 38,4	23 28 7,0 On Jan. 1, 1769.
Latitude	51 28 40					
	23 28 8,5 mean obliquity of ecliptic on Jan. 1, 1769.					

This last obliquity 23° 28′ 8″,5 is deduced in the manner used by Dr. Bradley, and is more to be depended on than the other 23° 28′ 7″,0 deduced from both solstices, on account of the less certainty of the lower refractions, from which, however, it only differs a second and a half.

Thus all the observations of the sun and circumpolar stars accord to 1″ or 2″ with the latitude of the Observatory settled by Dr. Bradley, making use of his refractions.

I shall now determine the latitude independent of Dr. Bradley's refractions, and infer the higher refractions at the same time, from a comparison of my observations of the apparent zenith distance of the equator before set down with Dr. Bradley's observations of the apparent zenith distance of the pole, both taken with the same excellent brass mural quadrant, in the same manner as Dr. Bradley deduced them from

from the apparent zenith diſtance of the equator obſerved with the iron quadrant compared with the apparent zenith diſtance of the pole obſerved with the braſs quadrant, according to the extract from the manuſcript in my poſſeſſion before cited.

The mean apparent zenith diſtance of the equator, by my obſervations of ſix years from 1765 to 1771, was related before 51° 27′ 29″,8. The mean apparent zenith diſtance of the pole was found by Dr. BRADLEY from 1750 to 1752 to be 38° 30′ 35″. Their ſum 89° 58′ 4″,8 taken from 90° leaves 1′ 55″2, the ſum of the refractions at the two zenith diſtances. Saying then, as 1′ 56″,7 the ſum of the refractions by Dr. BRADLEY's rule, to 1′ 55″,2 the ſum by obſervation, ſo are 1′ 11″,4 and 45″,3 the reſpective refractions at the two apparent zenith diſtances of the equator and pole by Dr. BRADLEY's rule, to 1′ 10″,5 and 44″,7 the two refractions at thoſe zenith diſtances, which added to them give the co-latitude 38° 31′ 19″,7, and the latitude 51° 28′ 40″,3. And as 1′ 56″,7 : 1′ 55″,8 :: ſo is 57″ the refraction at the apparent zenith diſtance 45° 3′ by Dr. BRADLEY : 56″27 the true refraction at that zenith diſtance, or not half a ſecond differing from Dr. BRADLEY's, but more to be depended on as deduced from obſervations made with the braſs quadrant only, and calculated from a parallax of the ſun nearer to the truth.

But if the apparent zenith diſtance of the pole be made uſe of, reſulting from a mean of 310 obſervations made with the braſs quadrant, according to Dr. BRADLEY's manuſcript, communicated by Dr. HORNSBY, from the whole of his obſervations from 1750 to 1753, *viz.* 38° 30′ 36″, the ſum of this and 51° 27′ 29″,8, the apparent zenith diſtance of the equator found by myſelf with the iron quadrant, or 89° 58′ 5″8 taken from 90° leaves 1′ 54″ 2, the ſum of the two refractions

at

at the pole and equator. Whence the refraction at the pole will be found in like manner as before 44″,3, and that at the equator 1′ 9″,9, and the latitude 51° 28′ 39″,7, and the refraction at the apparent zenith distance of 45° 3′ = 55″,8, which is 1″, 2 less than Dr. BRADLEY's determination, and 1″,2 greater than deduced from Mr. HAWKSBEE's experiment of the refraction of the air hereafter cited. It will be shewn in the sequel, that the latitude thus found does not at all depend on the truth of the total arc, but only supposes the instrument proportionally divided at the points answering to the pole and the equator.

From the whole then I conclude, that the latitude of the Royal Observatory at Greenwich is firmly established from Dr. BRADLEY's observations and my own at 51° 28′ 40″, probably without the error of a single second.

Let us now inquire into the latitude of the Royal Observatory at Paris. M. LE MONNIER, in the Memoires of the Royal Academy of Sciences for 1738, and in his *Histoire Celeste*, has examined into the latitude of the Royal Observatory at Paris, resulting from the observations of the principal French astronomers, and assuming the refraction at the height of the pole at Paris to be 50″, which is 2″ less than DOMINICO CASSINI's table gives, and the same which Dr. BRADLEY's rule gives, he finds the latitude of their Royal Observatory as follows:

		°	′	″
From the observations of M. PICARD	-	48	50	10
———————— of M. DE LA HIRE	-	48	50	12
———————— le Chev. DE LOUVILLE		48	50	8
———————— M. Maraldi	-	48	50	14

His

	°	′	″
His own observations in 1738, after examining and making an allowance for the error of the total arc of his quadrant - - - -	48	50	14
His further observations in 1740, making allowance for the error of the total arc of his quadrant, and considering the effect of the state of the air indicated by the thermometer upon the refractions	48	50	15
In the Memoires of the Royal Academy of Sciences for 1744, M. CASSINI DE THURY (the author of the memoir) *finds* from his own observations, with the same refractions - - - -	48	50	12
In the Memoires of 1755, the Abbé DE LA CAILLE, from a nice and accurate calculation of his observations made at the College of Mazarine, at Paris, and the Cape of Good Hope, deduces new tables of refraction suitable to each place, and states their respective latitudes, and thence that of the Royal Observatory at Paris - - -	48	50	14
Hence the ancient observations of M. PICARD, M. DE LA HIRE, and the Chevalier DE LOUVILLE give	48	50	10
The modern and more accurate observations of M. MARALDI, M. LE MONNIER, M. CASSINI DE THURY, and the Abbé DE LA CAILLE, give -	48	50	14;

which is now generally made use of by the French astronomers as the true latitude of their Royal Observatory; and from the near agreement of so many diligent observers and able astronomers cannot be supposed to differ above 2 or 3″ from the truth. The difference of this and 51° 28′ 40″, the latitude of the Royal Observatory at Greenwich above stated, is 2° 38′ 26″, the true difference of latitude of the two Observatories, which,

from

from what has been said of the observations on which the respective latitudes were founded, cannot be supposed to differ above 3 or 4″ from the truth. What then becomes of the uncertainty of 15″ supposed by the late M. CASSINI?

The same difference of latitude I find nearly from a comparison of my own observations of γ and β Draconis, taken with the zenith sector in 1768, with those of the Abbé DE LA CAILLE in 1750 and 1756, given in his *Fundamenta Astronomiæ*, after making the proper allowances for aberration, precession, and nutation, and correcting my observations by Dr. BRADLEY's refraction, and the Abbé DE LA CAILLE's by his table, and making allowance for the distance of the Abbé DE LA CAILLE's Observatory from their Royal Observatory; *viz.* 2° 38′ 25″,4 from γ Draconis, and 2° 38′ 26″,1 from β Draconis; the mean being 2° 38′ 25″,7, differing only 0″,3 from that stated above; but from Dr. BRADLEY's observations 2° 38′ 24″9, and 2° 38′ 27″,2, mean 2° 38′ 26″,0. It is too well known to astronomers to need my pointing out, that the best method of determining the difference of latitude of places, differing but little in latitude, is by such differences of zenith distances of stars passing near the zeniths, as the two above cited, observed at both places, in the same manner as the amplitude of the celestial arc is observed for finding the length of a degree of the meridian by comparison with geometrical measures.

The question now will be, upon what foundation was the late M. CASSINI's supposition of an uncertainty of 15″ in the latitude of Greenwich built? This appears evidently to have been upon a passage in the Abbé DE LA CAILLE's researches into the astronomical refractions and latitude of Paris, contained in the Memoires of the Royal Academy of Sciences for

1755,

1755, p. 578, 579, where M. DE LA CAILLE takes the differences of zenith diſtances of 14 ſtars obſerved by Dr. BRADLEY (in correſpondence to the ſame obſerved by himſelf at the Cape of Good Hope, for determining the moon's parallax in declination) publiſhed in the Memoires of the Royal Academy of Sciences for 1752, and the ſame obſerved by himſelf at Paris, after his return from the Cape, and correcting them for the difference of the refractions at the reſpective zenith diſtances, according to his own table of refractions, and the known apparent motions of the ſtars, finds the mean 2° 37′ 23″,9, which added to 48° 51′ 29″,3, his latitude at the College of Mazarine, gave him 51° 28′ 53″,2 for the latitude of Greenwich, exceeding Dr. BRADLEY's latitude by 13 or 14″.

Now the legitimacy of this concluſion depends upon a ſuppoſition that both inſtruments meaſured the true angle, or that their total arcs were juſtly laid off, and that the Abbé DE LA CAILLE's table of refractions is juſt. The firſt indeed has been proved with reſpect to Dr. BRADLEY's quadrant, but never has been attempted with reſpect to the Abbé DE LA CAILLE's ſextant; for the examination which the Abbé made of his inſtrument by parts for every 7°½ (ſee Memoires of the Royal Academy of Sciences for 1751 p. 405.), could not determine the error of the whole arc, as the difference from the truth might be inſenſible upon ſuch ſmall arcs, and the examination ſeems to have been intended to find the differences of theſe ſmall arcs from one another rather than from the true arc which they repreſent. We may therefore be allowed to doubt of the truth of this circumſtance. This doubt will be further ſtrengthened by ſeveral particulars which I ſhall adduce.

1. The apparent altitude of the pole at the Royal Obſervatory 48° 51′ 12″, reſulting from the Abbé DE LA CAILLE's

obſervations, exceeds 48° 51′ 4″ the mean of the obſervations of Meſſ. MARALDI, LE MONNIER, and CASSINI DE THURY, by 8″, and at the ſame time his refractions for that altitude exceed what they adopt by the ſame quantity. 2. His refractions are greater than all other tables give, DOMINICO CASSINI's, FLAMSTEED's, NEWTON's, BRADLEY's, MAYER's, SIMPSON's, and Lord MACCLESFIELD's. The latter I have by me in a manuſcript of Dr. BRADLEY, being what he uſed to correct his obſervations by, before he had been enabled to determine the refractions with the new mural arc. They were deduced from a braſs quadrant of 5-feet radius made by Mr. SISSON, ſtill remaining in the Obſervatory at Sherburn-Caſtle, and are the more to be eſteemed becauſe the diviſions of the inſtrument had been ſubmitted to the *ſtricteſt re-examination*, whereby, in the opinion of Dr. BRADLEY, it was probably rendered *as perfect in its kind as any extant, or as human ſkill could at that time produce.* See Dr. BRADLEY's Letter to Lord MACCLESFIELD, Phil. Tranſ. vol. XLV. p. 5. The refractions in this table are leſs than Dr. BRADLEY's by 2″,4 at the altitude of 45°, and 4″ at the altitude of 20°. MAYER's refractions agree almoſt exactly with Dr. BRADLEY's, and are entitled to much weight, having been determined by a 6-feet mural arc conſtructed by Mr. BIRD. 3. The refractions were found by the French Academicians at the polar circle, according to M. MAUPERTUIS's Book on the Figure of the Earth, to agree nearly with DOMINICO CASSINI's table. Hence it may be inferred, that the refractions in a warmer climate, as France, ſhould be leſs than according to the ſame table, and therefore much leſs than according to M. DE LA CAILLE's, and approaching to Dr. BRADLEY's, which are a little leſs than M. CASSINI's. 4. M. LE MONNIER, after his return from the polar circle, with a quadrant examined at the zenith and horizon, and after making allowance for the error thereby inferred in the total arc, ob-

ſerved a great many refractions of ſtars under the pole, with the ſtate of the thermometer, and ſometimes of the barometer alſo, as recorded in his *Hiſtoire Céleſte.* Theſe I calculated formerly, and found the refractions obſerved in very hot and very cold weather, compared together, to follow the ſame rate of increaſe and decreaſe, according to the changes of temperature, as Dr. BRADLEY has aſſigned; and, reducing the obſerved refractions to the mean temperature, I found them agree nearly with Dr. BRADLEY's. 5. The refractive power of the air about its mean temperature was carefully obſerved by Mr. HAWKSBEE, as related in his Phyſico-Mechanical Experiments, and the ratio of the ſine of incidence to that of refraction out of air into a vacuum found to be as 999736 to 1000000. Hence the aſtronomical refraction at the altitude of 45° ſhould be 54″,6, only 2″,4 leſs than Dr. BRADLEY's, and 2″ leſs than the ſame when his higher refractions are new calculated with the true parallax of the ſun, and 1″,2 leſs than I have before ſhewn to reſult from my obſervations of the apparent zenith diſtance of the equator compared with Dr. BRADLEY's of the apparent zenith diſtance of the pole, both taken with the ſame braſs mural quadrant, but 12″ leſs than the Abbé DE LA CAILLE's.

From all theſe facts, I think, I may be allowed to conclude, that the Abbé DE LA CAILLE's refractions are not juſt, but conſiderably too large; and, conſequently, as there can be no doubt of the care or diligence uſed by this aſtronomer in his obſervations and calculations, that the total arc of his inſtrument is too large for the radius, and, as I ſhall ſhew preſently, gives the meaſures of the zenith diſtances too ſmall.

But it may aſked, are then all the obſervations of this great aſtronomer, with their reſults, the fruit of ſo much labour and pains, to be conſidered as uncertain or lowered in their value in proportion to the error of his inſtrument? I am happy to anſwer,

anſwer, that the very ingenious method which he uſed of getting his refractions from the compariſon of the ſum of the apparent altitudes of the poles at Paris and the Cape with the ſum of the apparent zenith diſtances of ſtars paſſing the meridian between the two places, has fortunately, without his being aware of it, given him the refractions affected with the error of the arc of the inſtrument, and conſequently proper for correcting his obſervations; for, if the inſtrument be ſuppoſed ill divided, any error in the diviſions will naturally be thrown upon the refractions; and, if the total arc is too large for the radius, the ſtars will appear to approach the zenith by the error of the diviſions as well as the refractions, and the refractions in the table will come out too large, but ſtill ſuitable to the inſtrument, becauſe a correction is neceſſary to be added to the obſerved zenith diſtance, on account of the error of the inſtrument, as well as of the true refractions, and the table deduced from the inſtrument gives the ſum of the two corrections together, without determining them ſeparately.

Hence his table of refractions, though well adapted to his inſtrument, may be very unfit to be applied to any other. His latitudes of his obſervatories and his declinations of the ſtars will not loſe any of their certainty, at leaſt within the limits of the zenith diſtances meaſured by his ſector, *viz.* 60°. And this accounts for a circumſtance, at firſt ſight rather extraordinary, that his declinations of ſtars ſhould agree ſo nearly (generally within 5″ of Dr. BRADLEY's, as Dr. BRADLEY himſelf remarked) though his refractions made uſe of were ſo very different.

Having now ſhewn, that the Abbé DE LA CAILLE's refractions are too great, and only fit to be applied to his own inſtrument, it will be eaſy, by a juſt calculation, to reconcile the before

before mentioned zenith diſtances of 14 ſtars, obſerved at Greenwich by Dr. BRADLEY and by the Abbé DE LA CAILLE at the College of Mazarine at Paris, with the eſtabliſhed latitudes of the two Obſervatories, nearly; in doing which I ſhall claim the ſame right to correct Dr. BRADLEY's obſervations by his table of refractions, as I have allowed the Abbé DE LA CAILLE to be intitled to correct his obſervations by his table of refractions; which, I think, will be allowed me, after what I have ſaid of the manner in which the Greenwich refractions were deduced and the inſtruments made uſe of. The difference of latitude of the College of Mazarine and the Royal Obſervatory at Greenwich will then come out by the ſeveral ſtars, as follows; 2° 37′ 12″,7, 16″,0, 13″,8, 13″,7, 18″,4, 17″,7, 19″,7, 23″,2, 17″,7, 17″,0, 13″,9, 12″,4, 5″,6, 11″,9. The mean is 2° 37′ 15″,2 (or 8″,7 leſs than the Abbé DE LA CAILLE's reſult in his method of calculation, which I have ſhewn to be inadmiſſible) and added to 48° 51′ 29″,3, the latitude of the Abbé DE LA CAILLE's Obſervatory gives 51° 28′ 44″,5 for the latitude of the Royal Obſervatory at Greenwich, only 4″½ more than eſtabliſhed by Dr. BRADLEY's obſervations and my own; a ſufficient agreement, eſpecially conſidering that many of the ſtars were at great diſtances from the zenith, and that no account has been made of the temperature of the air at the times of the obſervations. The proper method, however, of ſettling the difference of latitude of two Obſervatories is by ſtars near the zenith, as I obſerved before; and the difference of the latitudes of the two Obſervatories of the College of Mazarine and Greenwich, by the Abbé DE LA CAILLE's obſervations of β and γ Draconis compared with mine, was 2° 37′ 10″,4, and compared with Dr. BRADLEY's 2° 37′ 10″,7, the firſt of which, added to the Abbé DE LA CAILLE's latitude, gives

51°

51° 28′ 39″,7, and the other 51° 28′ 40″, for the latitude of the Royal Obſervatory at Greenwich, exactly agreeing with that deduced immediately from the obſervations made at this place.

The ſame reſult nearly follows from M. CASSINI DE THURY's own obſervations of the zenith diſtance of the ſun at the ſummer ſolſtice of 1755, contained in the Memoires of the Royal Academy of Sciences for that year, compared with Dr. BRADLEY's, which latter was communicated to me by the late JOHN HOWE, Eſq.; for, by M. CASSINI's obſervations, the ſolſtitial altitude of the ſun's upper limb, corrected by the difference of refraction and parallax, according to DOMINICO CASSINI's table, which happens to agree with the ſame difference by my tables at this height, was 64° 53′ 36″, from which 15′ 47″ being ſubtracted for the ſemi-diameter of the ſun according to MAYER's tables, there remains 64° 37′ 49″, the true altitude of the ſun's center, and conſequently the ſun's true zenith diſtance 25° 22′ 11″. But the ſame was found from Dr. BRADLEY's obſervations, by my tables of refractions and the ſun's parallax, 28° 0′ 32″,8. The difference 2° 38′ 21″,8 or 2° 38′ 22″ is the difference of latitude of the two Obſervatories, which added to 48° 50′ 14″, the latitude of the Royal Obſervatory at Paris, gives 51° 28′ 36″ for the latitude of the Royal Obſervatory at Greenwich, or only 4″ leſs than before ſtated from the Greenwich obſervations, the difference lying the contrary way to that which the Abbé DE LA CAILLE carried the latitude of Greenwich, by improperly applying his own table of refractions to the Greenwich obſervations as well as to his own.

The Abbé DE LA CAILLE having, in the ſequel of his memoir, inferred the difference of latitude of Gottingen and the College of Mazarine from 22 ſtars obſerved by M. MAYER

with

with a 6-feet mural quadrant of BIRD's construction, correspondent to the same observed by himself, I shall make some remarks on this comparison, because it appears to me to afford a strong argument to shew that the Abbé DE LA CAILLE's refractions are too great; and that MAYER's, which agree with Dr. BRADLEY's, are just. The Abbé, after correcting the zenith distances of 22 stars observed at both places by his own table of refractions, finds the difference of latitude of Gottingen and Paris, by a mean, to be 2° 40′ 35″,1, which added to 48° 51′ 29″,3, the latitude of the College of Mazarine, gives him the latitude of M. MAYER's Observatory 51° 32′ 4″,4. He adds, that some observations of the pole star sent to him by M. MAYER would give the latitude of Gottingen 19″ less than he has established it, as just mentioned. Now I find, that if M. MAYER's observations of the pole star, as well as of the stars to the south of the zenith, be corrected by M. MAYER's table of refractions, and the Abbé DE LA CAILLE's observations by *his* table of refractions, the latitude resulting from M. MAYER's observations of the pole star will agree to 2″ with that resulting from the difference of latitude by the stars to the south; for subtracting 19″ from 51° 32′ 4″,4, the latitude which the Abbé DE LA CAILLE has assigned to Gottingen in the manner above-mentioned, there remains 51° 31′ 45″,4 the latitude which he found by the pole star; to which adding 52″,8, the refraction at the mean height of the pole star according to the Abbé DE LA CAILLE, the sum 51° 32′ 38″,2 must be the apparent height of the pole by Mr. MAYER's observations, which diminished by 45″,6, M. MAYER's refraction, gives the true latitude of M. MAYER's Observatory 51° 31′ 52″,6. But by the difference of the Abbé DE LA CAILLE's and M. MAYER's zenith distances of the 22 stars to

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the ſouth, corrected each by their own table of refractions, I find the difference of latitude 2° 40′ 32″,0, 28″,9, 25″1, 27″,9, 22″,6, 28″,3, 32″.0, 26″,7, 27″,2, 24″,2, 25″,4, 31″,3, 23″,2, 23″,6, 28″,9, 18″,4, 16″,7, 22″,0, 23″,8, 21″,1, 27″,3, 27″3, the mean of which is 2° 40′ 25″,6, which, added to 48° 51′ 29″,3, the latitude of the College of Mazarine, gives the latitude of Gottingen 51° 31′ 54″,9, or only 2″,3 more than I have deduced above from M. MAYER's obſervations of the pole ſtar rightly corrected, and only 0″,9 leſs than is ſet down in M. MAYER's tables, which he expreſsly ſays, p. 48. of the precepts to his ſolar and lunar tables, publiſhed by myſelf for the Commiſſioners of Longitude in 1770, was deduced from his own obſervations.

I have before, when I ſhewed the Abbé DE LA CAILLE's refractions to be conſiderably too great, at the ſame time vindicated them as fit for his inſtrument, becauſe he deduced them in a manner which gave him the apparent elevation of objects above their true place by the ſum of refraction and the error of his inſtrument, if his inſtrument meaſured the zenith diſtances too ſmall, as I had concluded it did. The like remark may be applied to Dr. BRADLEY's table; for his refractions at the pole and equator, having been determined with one and the ſame quadrant, at one time turned to the north to obſerve the apparent zenith diſtance of the pole by means of the polar ſtar and other circumpolar ſtars, and afterwards to the ſouth to obſerve the apparent zenith diſtance of the equator, in the manner before explained, muſt neceſſarily be the true refractions, if the inſtrument meaſured the true angle; and the ſum or difference of the true refractions and the errors of the inſtrument for theſe zenith diſtances, in caſe the inſtrument did not meaſure the true angle; and therefore equally proper to correct his obſervations, whether the total arc was juſt or not.

 Moreover,

Moreover, from the two refractions thus found at the equator and pole, the refractions of the circumpolar ſtars at their paſſing the meridian above the pole were computed by Dr. BRADLEY, from the hypotheſis that the refractions at conſiderable altitudes are as the tangents of the zenith diſtances; which rule is pretty accurately true with reſpect to the real refractions, and would vary but little from the truth for the apparent refractions, which would be the ſum or difference of the true refractions and the errors of the arc, in caſe the total arc erred from the truth by a very ſmall quantity, not exceeding 10 or at moſt 20 ſeconds. The obſerved zenith diſtances of the ſtars above the pole being corrected by the refractions thus computed, and ſubtracted from the known co-latitude, gave their true diſtances from the north pole, which added to the co-latitude gave their true zenith diſtances under the pole; and this diminiſhed by the obſerved zenith diſtance would give the refraction under the pole, or the ſum or difference of the refractions and the errors of the inſtrument belonging to their reſpective zenith diſtances; and thus his whole table would exhibit the ſum or difference of the true refraction and error of the inſtrument. Hence the latitude of Greenwich eſtabliſhed by Dr. BRADLEY, with his quadrant, as well as the latitudes of the Obſervatories at the College of Mazarine and the Cape of Good Hope, ſettled by the Abbé DE LA CAILLE with his ſextant, and the declinations of the ſtars and the obliquity of the ecliptic found by both will be very near the truth, independent of the juſtneſs of the total arcs, although their reſpective refractions may be ſuitable only to their own particular inſtruments. But, for the reaſons before given, I apprehend, the Abbé DE LA CAILLE's refractions to be much too large, and Dr. BRADLEY's to be very near the truth.

I shall now close my enquiry into the latitudes of Greenwich and Paris, and Dr. BRADLEY's and the Abbé DE LA CAILLE's refractions, by a remark naturally arising from my comparison of, and endeavours to reconcile, their observations, which I desire to submit to the consideration of astronomers, it not having, that I know of, been made before; that a table of refractions should be made for every vertical instrument from observations made with itself turned alternately north and south; and that the table, so made, applied to observations made with it, will give the true zenith distances, whether the total arc of the instrument be accurately just, or affected with a small error, or however unequally it be divided below the pole, provided the divisions are equal between themselves in the part of the instrument lying between the equator, the zenith, and the pole.

It remains to give some account of the longitude of Greenwich, or rather of the difference of meridians of Greenwich and Paris, in reply to the late M. CASSINI's doubts on the subject. This had been settled by Dr. BRADLEY at 9′ 20″, as he informed me himself, and that he had deduced it from eclipses of Jupiter's first satellite observed at both places, and that he had found it come out the same both from the immersions and emersions. This quantity had been inserted in the table of latitudes and longitudes of places, prefixed to Dr. HALLEY's tables, on the authority of Dr. BRADLEY, so long ago as the year 1749, the date of the publication of those tables, and was generally admitted by astronomers till the year 1763, when the late Mr. JAMES SHORT, F. R. S. computed it from the four transits of Mercury over the sun in 1723, 1736, 1743, and 1753, observed at Paris, London, and Greenwich, to be 9′ 16″. See Philosoph ical Transactions, Vol.LIII. p. 158. In

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the year 1776, I requested the late Mr. WARGENTIN, the learned Secretary of the Royal Academy of Sciences at Stockholm, and Author of the improved Tables for computing the Eclipses of Jupiter's Satellites, who collected observations of them from the principal Observatories of Europe, in order to the further improvement of the tables, to inform me what difference of meridians of Greenwich and Paris resulted from my last ten years observations of the eclipses of the first satellite of Jupiter compared with those made by M. MESSIER at Paris. In the answer which he favoured me with, inserted in the Philosophical Transactions, Vol. LXVII. p. 162. he set down the result of the comparisons of eight corresponding immersions and nine emersions observed on both parts, by myself and M. MESSIER, from which he deduced the difference of meridians of the Royal Observatories of Greenwich and Paris 9′ 35″. By two corresponding immersions and nine corresponding emersions, observed at both Royal Observatories, he found 9′ 21″. From the observations made between 1761 and 1764 he found 9′ 28″. By the observations made before 1700, 9′ 21″. And, from a comparison of mine and the Parisian observations, with the intermediate help of his own made at Stockholm, 9′ 26″: and from the whole he inferred the difference of meridians to be 9′ 25″.

Twelve years having elapsed since Mr. WARGENTIN's comparison, I was desirous to see what would result from the further observations made during that time, and applied to the Comte CASSINI, the respectable heir of the late M. CASSINI DE THURY and his successor at the Royal Observatory, and to the celebrated M. MESSIER, to favour me with such of their observations of the eclipses of the first satellite of Jupiter as had been made correspondent to mine. These they immediately sent me in the

the moſt obliging manner, by which I am enabled to make further inferences concerning the difference of our meridians, as exhibited in the two following tables; in reference to which it is to be underſtood, that all the obſervations at Greenwich were made with a 46-inch achromatic teleſcope of 3,6 inches aperture, except a few otherwiſe noted as obſerved with a 6-feet Newtonian reflector, whoſe aperture is 9,4 inches; and once with a 2-feet Gregorian reflector, whoſe aperture is 4,5 inches; and once with an 18-inch Gregorian reflector, with an aperture of 4,4 inches, furniſhed with new metals of Mr. EDWARDS's brilliant compoſition, deſcribed in the Appendix to the Nautical Almanac of the preſent year, which reflects as much of the incident light as an achromatic teleſcope tranſmits; and that in making out the columns, intitled difference of meridians corrected, I have ſubtracted 7″ from the immerſions and added as much to the emerſions obſerved with the 6-feet reflector, and added 13″ to the immerſions and ſubtracted as much from the emerſions obſerved with the 2-feet reflector, to reduce them to what they ſhould have been probably obſerved at with the 46-inch achromatic teleſcope, and added 5″ to the time of the emerſion obſerved on Sept. 5, 1784 at the Royal Obſervatory at Paris, with a 5-feet reflector of DOLLOND, to reduce it to the 3½ feet achromatic teleſcope.

Difference

Difference of meridians of the Royal Obſervatories at Greenwich and Paris, by obſervations of eclipſes of Jupiter's firſt ſatellite, obſerved at both places.

By Immerſions.	Difference of meridians.	Diff. of meridians corrected.	Circumſtances of the obſervations at Greenwich.	Circumſtances of the obſervations at the Royal Obſervatory at Paris.
	′ ″	′ ″		
1779, Jan. 11	9 22	9 29	6 F.	Limbs undulating.
18	9 38	9 45	6 F.; air a little hazy.	Air very clear.
1780, Jan. 14	9 5	9 5	Air very clear.	
Mar. 18	9 16	9 16	Air very clear.	Air hazy.
25	9 8	9 8	– –	Air a little hazy.
1785, Oct. 1	9 11	9 11	In contact with Jupiter's body.	A little hazy.
Mean of 6 imm.	9 17	9 19		

By Emerſions.	Difference of meridians.	Diff. of meridians corrected.	Circumſtances of the obſervations at Greenwich.	Circumſtances of the obſervations at the Royal Obſervatory at Paris.
	′ ″	′ ″		
1773, Nov. 1	9 10	9 3	6 F. Air a little hazy.	
1775, Feb. 15	10 44	10 37	6 F. Twilight.	Air very clear.
Mar. 17	9 43	9 36	6 F. Air a little hazy.	Air very clear.
1778, Mar. 13	10 46	10 46	—Bright moonſhine.	
Apr. 12	9 46	9 46	Air hazy.	
1779, Mar. 30	10 43	10 43	– –	Air hazy.
Apr. 1	9 56	10 9	2 F. reflector. Air very clear.	
1781, May 24	9 49	9 49	Air very clear.	
31	9 8	9 8	– –	Air very clear.
1782, Aug. 29	9 27	9 27	Air very clear.	
1783, Aug. 2	8 54	8 54	Air very clear.	The ſat. very near Jupiter's diſk.
Oct. 26	9 45	9 45	Air very clear, but Jupiter low.	Limbs undulating much.
1784, Sept. 5	9 4	9 9	Air very clear.	5-feet reflector by DOLLOND, magnifying 450 times.
1785, Nov. 9	9 26	9 26	18 inch reflector, new metals.	
16	9 45	9 45	– –	Hazy.
Mean of 15 emer.	9 44,4	9 42		
Mean of 6 imm.	9 17	9 19		
Mean of both means	9 31	9 30½		

Before

Before the 24th of May, 1781, a 3½-feet achromatic teleſcope of DOLLOND, of 42 lines aperture, that was but an indifferent one, was made uſe of at the Royal Obſervatory at Paris. From that time a very good one was employed of the ſame ſize and aperture.

Difference of meridians of the Royal Obſervatory of Greenwich and the Hôtel de Clugny at Paris, 2″ of time Eaſt of the Royal Obſervatory, deduced from obſervations of eclipſes of Jupiter's firſt ſatellite obſerved at both places.

By Immerſions.	Difference of meridians.	Diff. of meridians corrected.	Circumſtances of the obſervations at Greenwich.	Circumſtances of the obſervations at Hôtel de Clugny.
	′ ″	′ ″		
1775, July 15	9 55	9 55	– –	Air very clear.
Aug. 7	9 6	9 13	6 F.	Air very clear.
1776, Sept. 10	9 22	9 22	Jupiter a little hazy.	
1777, Sept. 6	9 33	9 33	– –	Air very clear.
Nov. 7	9 23	9 23	Air hazy.	Air very clear.
1779, Jan. 11	10 18	10 25	6 F.	Air very clear.
18	9 23	9 30	6 F. air a little hazy.	Air very clear.
Dec. 22	8 10	8 10	Air very clear.	A little hazy.
1780, Jan. 7	9 39	9 39	Air very clear.	Air very clear.
14	9 40	9 40	Air very clear.	Air very clear.
Mar. 18	9 2	9 2	Air very clear.	Air very clear.
25	9 24	9 24		
1781, Feb. 26	8 48	8 48	♃'s limb undulates.	
Mar. 5	8 59	8 59	– –	Air very clear.
Apr. 13	9 16	9 16	– –	Jupiter ill defined.
1783, July 8	9 58	9 58	– –	Air very clear.
1786, Sept. 4	9 19	9 19	Air very clear.	
Dec. 30	9 31	9 31	Air very clear.	Very hazy.
Mean of 18 imm.	9 22,5	9 23,7		

By

By Emersions.	Difference of meridians.	Diff. of meridians corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Hôtel de Clugny.
	′ ″	′ ″		
1775, Feb. 15	9 31	9 24	6 F. Twilight.	Air very clear.
22	9 1	8 54	6 F.	Air very clear.
Mar. 17	9 13	9 6	6 F. Air a little hazy.	Hazy.
1776, Jan. 26	9 15	9 15	— —	
1777, Feb. 4	8 51	8 51	Air very clear.	Air very clear.
Mar. 24	9 13	9 13	Air very clear.	Air very clear.
31	9 22	9 22	Air very clear.	
1778, Feb. 25	8 48	8 48	— —	Air hazy.
Mar. 13	9 15	9 15	— —	A little hazy.
Apr. 5	9 17	9 17	Air hazy.	Air very clear.
12	10 11	10 11	— —	Air very clear.
May 21	9 29	9 22	6 F.	Air very clear.
1780, Apr. 19	9 28	9 28	Air very clear.	
May 28	9 37	9 37	— —	Air very clear.
1781, May 31	9 10	9 10	— —	Air a little hazy.
June 16	9 25	9 25	— —	Air a little hazy.
1783, Aug. 2	9 23	9 23	Air very clear.	Air very clear.
25	9 40	9 40	Air very clear.	
Oct. 3	9 30	9 30	Air very clear.	Air hazy.
1785, Nov. 9	9 14	9 14	18-inch reflector, new metals.	
18	9 12	9 12	Air very clear.	
1786, Jan. 3	9 58	9 58	Air a little hazy.	Air hazy.
Mean of 22 emer.	9 22	9 20,7	M. Messier's achrom. telescope is of $3\frac{1}{2}$ feet focus, 40 lines aperture, with magnifying powers of 70 and 140.	
Mean of 18 imm.	9 22,5	9 23,7		
Mean by both	9 22	9 22		
Royal Observ. west of Hôtel de Clugny	—2	—2		
Diff. of merid. of the Royal observatories	9 20	9 20		

Hence the difference of meridians of the two Royal Observatories, by the observations made in the Royal Observatories themselves, is 9′ 30″; and by the observations made by

M. MESSIER, at the Hôtel de Clugny, and reduced to the Royal Obſervatory is 9′ 20″. The mean of both reſults is 9′ 25″. But if greater weight be given to the latter determination than to the former in the ratio of 2 to 1, on account of the ſeries of M. MESSIER's obſervations being the moſt complete, the difference of meridians will be 9′ 23″.

M. DU SEJOUR, in the Memoires of the Royal Academy of Sciences for 1771, found 9′ 20″, as well from the beginning as end of the ſolar eclipſe of 1769. M. MECHAIN, the learned editor of the *Connoiſſance des Temps*, informs me, that from the immerſions of Celeno and Maia at the moon's limb, on March 5th laſt year, he has found by calculation from M. MESSIER's obſervations compared with mine 9′ 19″,9 and 9′ 17″,9, or by a mean 9′ 18″,9; but, by his own obſervations compared in like manner, he makes it a little more than 9′ 20″. He regulated his clock by correſponding altitudes; but M. MESSIER corrected his by a tranſit inſtrument, which, however, has no meridian mark. For the preſent, I infer, we may take the difference of meridians 9′ 20″, as being within a very few ſeconds of the truth, till ſome more occultations of fixed ſtars by the moon, already obſerved, or hereafter to be obſerved, in favourable circumſtances, and carefully calculated, ſhall enable us to eſtabliſh it with the laſt exactneſs. To collect and calculate ſuch obſervations I have not leiſure at preſent; but the field of calculation is equally open to the celebrated aſtronomers of Paris, the obſervations made at this place being now publiſhed annually.

The extenſive geometrical operations recommended by the late M. CASSINI DE THURY, and commenced under the direction of Major-general ROY, F. R. S. by his exact meaſure of a baſe on Hounſlow-Heath, may alſo, when completed, determine

mine the difference of meridians of Greenwich and Paris to great exactneſs. But they do not ſeem to me likely to throw any new light on the difference of latitude of the two Obſervatories, becauſe the uncertainty we are ſtill under about the true figure and dimenſions of the earth, and the irregular attractions ariſing from the irregular external figure and unequal denſity of the internal parts of the earth would prevent us from drawing any accurate concluſions, or ſuch as we could confide in, from thoſe geometrical meaſures, with reſpect to ſo large a quantity as 2° 38′ 26″ the difference of latitude; and, at all events, it muſt be leſs exact, as it is leſs direct, to determine the difference of latitude of two places from the meaſured diſtance of the two parallels compared with the length of a degree in the intermediate latitude, inferred from former meaſures of degrees, which were themſelves determined with the help of aſtronomical obſervations, than to infer it from the immediate aſtronomical obſervations made at the two obſervatories, in the manner I have already deduced it.

NEVIL MASKELYNE,
Aſtronomer Royal.

Greenwich,
February 21, 1787.